Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-75-RC1, 2018

Climate of the Past Discussions
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Interactive comment on "Extreme lowering of deglacial seawater radiocarbon content is recorded by both epifaunal and infaunal benthic foraminifera" *by* Patrick A. Rafter et al.

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Received and published: 16 August 2018

Rafter et al. present a new record of deglacial intermediate water radiocarbon from the southern tip of Baja California. This provides a direct test of the fidelity of the nearby (California Undercurrent, CU) Marchitto et al. record, which was originally proposed to track the deglacial release of aged carbon from the deep ocean, hypothetically via the Southern Ocean and AAIW. The CU result has been questioned because of the lack of a similar signal in other 'expected' locations along the AAIW flow path, and because of the inability of a box model to simulate such a strong signal in the face of mixing with better equilibrated waters. It has been suggested that the CU record may suffer from

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artifacts, or that it may record a local release of geologic carbon.

The present record has two principal strengths: it has wood-based 14C age control, and it compares different species of benthic foraminifera, including an epifaunal taxon. The general agreement with the CU record therefore provides a strong argument against some of the hypothetical artifacts that have been previously invoked. I think this is an important result, and I support publication after substantial revisions. (Parenthetical numbers below refer to page/line numbers.)

First, I suggest that the authors place greater emphasis on the novelty and robustness of their wood-based age model. Only very recently has another such wood-based record been published, claiming to be "the first high-resolution record that is free of the dating uncertainties common in marine sediment records" (Zhao and Keigwin, 2018, Nature Communications). The greater emphasis could begin with the title, by appending something like "in a wood-dated core" to the end of it. The value of wood can also be emphasized in the Introduction, where the uncertainties of foram radiocarbon work are enumerated (beginning at 2/33). The first and foremost paragraph there should be about calendar age control, including uncertainties in planktic foram habitat and reservoir age, and (in the case of the CU record) assumptions about temporal correlations to other records. Given the novelty of the wood age model, I would also like to see a more explicit presentation of the rejected dates, including a figure. I do not have an intuition for how wood should behave in a marine core. Zhao and Keigwin suggest that wood will only float for months, and is hence quickly buried; but their core had a much higher sedimentation rate than the present study, so I wonder if the greater number of rejections here might be related to residence time at the seafloor before burial, or its lack of proximity to a river mouth? The accepted dates in Fig. 3 look very nice stratigraphically, but I'd like to see what the five rejected dates look like. The principal detail given is that they were older than coexisting foraminifera. The statement about "macrofauna consumption" (6/5) is nebulous, and it is not cleared up later (9/1) without the aid of a figure.

A key assigned date in core ET97-7T is based on a color change that was wood-dated in the very nearby core LPAZ-21P. This assignment produces a somewhat alarming increase in the sedimentation rate of ET97-7T, in comparison to LPAZ-21P. As such, it needs to be better defended. The color change is described as abrupt in LPAZ-21P but no description is given for it in ET97-7T. It appears to coincide with a hiatus (or sharp drop in sed rate), so nailing the true age of the transition in the core may be difficult. Based on my experience with other Baja cores, I suspect the color change occurs at the start of the Holocene (∼11.7 ka, admittedly subject to assumptions about temporal correlation with Greenland), but mud of that exact age may well be missing. I guess that the 12.1 ka date in LPAZ-21P sits in the lighter colored mud, meaning the depth assigned to that age in ET97-7T (and hence the mud below it) is also light? In that case, that depth is likely no younger than 11.7, a hypothetically small uncertainty that would not make the high sed rates go away. In any case, reflectance records or core photos would help assure me that this age assignment is valid. Because this interval of ET97-7T is not constrained by wood dates, I suggest that the benthic dates in the lower panel of Fig. 3A be color-coded by core, so that one can see where the ET97-7T analyses sit.

The authors devoted considerable effort to counting foram abundances "to account for bioturbation" (3/23) but I do not see where they actually did very much with the abundance data. They compare inter-species ages overall and on abundance maxima (8/6), but n for the latter is unfortunately small. D14C values from abundance maxima are plotted differently in Fig. 5; can the authors say anything about whether off-maxima values are consistent with bioturbation? Since bioturbation is one of the chief potential bogeymen here, I think it deserves its own section in the Discussion, namely "Can bioturbation explain the low deglacial D14C?" A key point is that the wood dates do not support a wholesale bioturbational artifact (and likewise the planktic dates in Lindsay et al. 2015). Perhaps bioturbation can explain some of the scatter in the benthic dates but not the overall deglacial pattern? It is worth noting explicitly that the noise is likely affected by the low sed rates in comparison to the CU site. Given this noise, I'm not convinced that the lack of higher D14C values forming the middle of the "W" (10/3) is significant.

The interspecies age offsets are interesting, and sometimes alarming. The CU dates are described as "mostly mixed benthic species" (8/20) but that's misleading: we had 29 Uvigerina, 10 Bolivina, and 21 mixed (please clarify in text and Fig. 5). The new cores' interspecies offsets are discussed in terms of 14C age, but only shown as D14C. I suggest that the first data figure should show the 14C ages for each core, by species, versus depth in core (probably it could be combined with the wood date figure that I request). That would be the clearest demonstration of the interspecies offsets, and the potential for bioturbational mixing to explain some of them. It might also be nice to plot the D14C in Fig. 5E as species-color-coded symbols rather than a single black line. The next question is, if not caused by bioturbation, could interspecies differences be real? Are the quoted average age differences (8/4 and Table 2) actually significant? Pore waters are raised in the Intro (3/5), but what direct (non-foram) evidence is there that shallow pore waters can be significantly different age than bottom waters (citations?). Is it plausible that diagenesis (section 4.2) could affect taxa differently due to, e.g., surface texture? On the topic of diagenesis, it seems to me that another argument against that being a dominant effect is that if diagenetic old carbon were somehow migrating from deeper pore waters, it would likely have a pretty different age between the CU site and the present site, given the very different sed rates (cf. 11/24).

The manuscript mostly punts on the question of where the old carbon is coming from (Conclusions), and I don't fault them for that because it remains a puzzling problem. I think diagenesis should not be raised again at the start of this section (12/3) because it was just addressed in the previous section. I think the 'similarity' of distant sites (Southern Ocean, North Atlantic) is overstated; really only the Arabian Sea (Bryan et al., 2010) looks very similar. The lack of any deglacial signal at some key sites, like Chile (De Pol-Holz) and now Colombia (Zhao and Keigwin), could be reiterated. Consider citing another paper just out, Du et al. (2018) Nature Geoscience, which discusses a possible flushing of the deep Pacific during deglaciation. Rafter et al. are correct to point out that geologic carbon would need to be buffered. This is true not only if the geologic release were "global-scale" (12/21) but even if it were localized to places like Baja California: the DIC addition to local seawater would need to be huge, causing dissolution if not buffered (see Lindsay et al. 2016, p. 1113, for a rough calculation). Assuming buffering is possible, could the lack of a signal at some intermediate sites (12/18) just be because they are not near ridges/vents?

There is a slight muddling of DD14C and age in this manuscript. Part of the CU DD14C discussed in the intro is simply due to higher atmospheric D14C and not water mass aging (cf. 2/14) (see Lindsay papers for magnitude). The present wood-benthic age difference could be caused by intermediate water aging but not by higher atmospheric D14C (6/22), which affects un-normalized DD14C but not age differences.

Additional comments by line:

2/9: The CU core has only one DD14Cvalue >500 per mil; say >400 instead.

2/20: Cite Broecker and Barker somewhere, but not here: they did not mechanistically link the deglacial 14C drop to the CO2 rise, but rather were not convinced of the sign of the abyssal reservoir's impact on pCO2.

2/24: "Equal to" is unlikely due to mixing ala Hain. Stick with "lower than" and say why.

2/25: "Lower" than deglacial intermediate waters, or lower than today at those locations?

2/29: Clarify that these are places where the signal would be expected if it was carried by AAIW (the signal would not be expected in ALL intermediate depth waters).

2/30: Is this Hain critique different from the mixing argument two sentences before? If so, briefly explain.

3/10: Note that this is largely attributed to the mysterious Pyrgo problem.

3/13: I'm not sure "infrequently taken into consideration" is fair. Everyone "considers" biuoturbation, but we infrequently quantify it?

3/19: I don't really see this paper as a "wide-ranging test of the fidelity of the benthic foraminifera D14C proxy." It is more directly a test of the Marchitto result. I think the fidelity of the proxy will remain pretty core-dependent.

4/6: What size fraction was counted?

4/27: Are these numbers mass of carbon or mass of CaCO3/wood?

8/15: Holocene D14C is noted to be similar to modern, but isn't that forced to be the case (at least for Uvigerina, which dominates the observations) due to using Uvigerina for the Holocene age model?

9/16: "Around 13-kyr" is not precise enough for the CU d18O drop. There are step changes at the start of the Bolling (14.6, halfway to Holocene values) and end of YD (11.7). Caveat: those dates are age model dependent. But it looks like you have the end-YD jump, with a muted/smoothed Bolling earlier.

9/29: This statement about interspecies age offsets being modest does not match the previous discussion nor the Table. Am I misunderstanding?

10/14: Not sure why van Geen is cited here?

10/15: It sounds like you're talking about transporting sand-sized benthic forams from one site to the other (at the same water depth, not downslope), which is far-fetched. Clarify what you mean here.

11/6: Say why you think Pyrgo is so old (or say that it's not known why).

12/11: Clarify that the unrealistic bit about alkalinity (in Hain's model) is that alkalinity gets trapped in the glacial (not deglacial) deep ocean at the expense of the surface, hence raising pCO2.

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Figure captions: Please provide descriptions for the different symbols, so the reader does not have to search through the text to interpret figures.

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